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BRIEFER ARTICLES.

FERTILIZATION IN THE SAPROLEGNIALES.

TROW[1] has reaffirmed his conviction that a sexual act is present in the water molds. His paper gives in detail the results of an investigation of *Achlya DeBaryana* and *A. polyandra* in comparison with my own study of oogenesis in Saprolegnia.[2] Much of his paper is devoted to a defense of his earlier work and to sharp criticisms of the skepticism of myself and others.

A priori there is of course no reason why the Saprolegniales should not show examples of sexuality; the discussion has only been on the evidence that TROW presented in his paper of 1899.[3] This evidence consisted in the varying number of nuclei present in the oosphere and oospore, expressed by TROW in the ratio 1:2:1, and certain figures of male gamete nuclei within the egg which did not seem to me clear enough to be convincing and appeared capable of other interpretations. I found that binucleate eggs, young and old, were "quite common" in an apandrous form of *Saprolegnia mixta*, and that by the methods of oogenesis bi- and trinucleate eggs might be expected in any of the Saprolegniales. Indeed all investigators of this group, HUMPHREY, HARTOG, and TROW himself, have known and described binucleate eggs. TROW in his last paper (p. 547) refers to the binucleate eggs of my form as of "occasional occurrence" and "in all probability of a pathological character." I stated expressly that they were "quite common" and I cannot see the slightest reason for suspecting them to be pathological. The cultures appeared perfectly healthy and the phenomenon was not infrequent as TROW implies. For these reasons I held that the number of nuclei in the ratio of 1:2:1, ordinarily accepted as evidence of fertilization when found successively in young and older stages of eggs and oospores, was insufficient for the establishment of sexuality in the Saprolegniales. To my mind proof is only furnished by clear and unmistakable instances of fusion between eggs and antheridial filaments and with the male nuclei actually *en route* to the female at the point of fusion and within the egg. Few botanists would have been satisfied I think with TROW's evidence on the latter point in his paper of 1899. I regarded his views as "unproven and improbable" in the face of the mass of observations upon which most botanists have generally agreed that the Saprolegniales were apogamous. I still consider myself quite justified in taking this attitude, which I tried to present with due consideration to TROW as possibly being mistaken in his interpretation of some very difficult conditions. He seems to have taken my expressions as controversial, which I tried hard to guard against, and has certainly answered them in that spirit. One result at least has been an account of fertilization in *Achlya DeBaryana* which in detail of description and clearness of figures is quite a different sort of contribution from that of 1899.

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[1] TROW, A. H., On fertilization in the Saprolegnieae. Ann. Botany **18**:541–569. *pls. 34–36.* 1904.

[2] DAVIS, B. M., BOT. GAZETTE **35**: 233. 1903.

[3] TROW, A. H., Ann. Botany **13**:131. 1899.

Achlya DeBaryana furnished TROW the most complete series of stages of fertilization. In his *figs. 23–26* the antheridial filaments are united with the eggs by broad strands of protoplasm, and nuclei lie in some cases almost exactly at the points of fusion, either as though they had just entered the egg or were about to do so (*fig. 23*). The figures (especially *24* and *25*) even show that peculiar structure of the protoplasm which indicates a flow from the antheridial filaments into the eggs, conditions which have been illustrated many times in accounts of fertilization among the Peronosporales. These statements give us what we have a right to expect of any account of fertilization, and especially when the technical conditions are so complicated as in the Saprolegniales. They seem to me fundamental to the present problem, and the establishment of the ratio of nuclei in oosphere and oospore as 1:2:1 merely a corollary to the main proposition. If TROW will exhibit his preparation showing these protoplasmic fusions of antheridial filaments with eggs at scientific meetings in Great Britain or elsewhere he is not likely to have any difficulty in establishing his contention.

Of greater interest to me than the evidence of fertilization is TROW's account of a second mitosis in the oogonium and the origin of the aster-like structures which I discovered in Saprolegnia and considered coeno centra. During the second mitosis, according to TROW, two centrosomes with radiations appear at the poles of the spindle in anaphase, structures which were not present in the first mitosis. Some of these increase rapidly in size and become the centers of the egg origins, each one being accompanied by the nucleus that lay beside it, after anaphase of the second mitosis. Relatively few of the nuclei in the oogonium pass through this second mitosis, the remainder degenerating, and TROW believes that some of the daughter nuclei of this division with their asters also break down. The surviving asters with their accompanying nuclei become the centers around which the cleavage of the protoplasm proceeds to form the eggs. Trow calls the egg-asters "ovicentra." He believes that the number of chromosomes shown in the first mitosis is reduced by half to form four in the second, so that the latter is a reduction division in oogenesis.

Trow's account of the development of his ovicentra is fundamentally different from mine. I regarded them as simply representing dynamic centers of the protoplasm without any relation to mitotic phenomena, centrosomes, or asters, and as strictly comparable to the coenocentra of the Peronosporales. Trow considers them centrosomes in an aster (astrospheres) appearing at a final reduction division (second mitosis) in the oogonium. The situation has become very much complicated. If ovicentra are the same as coenocentra, why have not the latter been found associated with mitotic figures in the Peronosporales, where the structures are large and have been much studied? Is it possible that we have a new structure in the Saprolegniales? I am not willing to give up my view of oogenesis, although I can at once see the necessity of further investigation in both the Saprolegniales and Peronosporales. I do not think that Trow has presented enough evidence or treated the subject sufficiently with reference to the Peronosporales to be convincing. His theory is based, so I understand, chiefly upon the sections from one oogonium. There are some interesting questions suggested by the group of figures numbered *18*, which one hesitates to ask without seeing Trow's preparations. But why are not the asters present at metaphase, the time when such structures are generally most conspicuous? And again, why are the asters sometimes so far away from the poles at anaphase (*fig.* 18^8)? Is it possible that the "anaphase" is simply a resting nucleus drawn out towards a coenocentrum and attracted by it, as is the habit in the Peronosporales? These questions are not put in a captious spirit, but by one who is deeply interested in the problem and knows the difficulty of the investigations. Trow has advanced a most interesting hypothesis, which he must expect to be keenly scrutinized.

"The sperm nucleus soon after its entry into the oosphere acquires a distinct centrosome and astrosphere." This seems to the writer the most remarkable announcement in Trow's paper. Neither the origin nor the fate of this structure was followed. It looks exactly like an ovicentrum. The male and female nuclei and their accompanying astrospheres remain quite apart in the egg until the astrospheres disappear. In the meantime the oospore gradually fills with reserve material and the wall thickens. While these processes are under way the gamete nuclei fuse. The structure of the egg when it contains two astrospheres with the gamete nuclei is of great interest, and this phase of the subject is worthy of extended study. I have seen and figured eggs in Saprolegnia with two coenocentra, each accompanied by a nucleus, when it was evident that the two were sister structures included within the same egg origin.

It is evident that very much more work must be done both in the Saprolegniales and Peronosporales before some of the points suggested by TROW'S paper will be established. An ovocentrum arising from an aster associated with a reduction mitosis is a very different sort of conception from our present idea of the coenocentrum in the Peronosporales. The same sort of structure is also reported to appear suddenly beside the sperm nucleus after its entrance into the oogonium. It seems hardly conceivable that similar structures could behave in such different ways or could hold relationships to the coenocentra of the Peronosporales. Of course my own view of the essential agreement of the Saprolegniales and Peronosporales in processes of oogenesis around coenocentra must await the results of future investigations.—B. M. DAVIS, *The University of Chicago.*

THE SEXUAL ORGANS AND SPOROPHYTE GENERATION OF THE RHODOPHYCEAE.

OLTMANNS'S theory that the structure developed from the fertilized carpogonium in the Rhodophyceae represents a sporophyte generation has received substantial confirmation in recent cell studies of WOLFE[1] on Nemalion. This investigation also makes an important contribution to our knowledge of the sexual organs of the red algae, establishing some complications of structure that are likely to prove very general in the group, and quite changes our conception of their morphology. Their possible relations to the sexual organs of the lichens, Laboulbeniaceae, and even to the Uredineae is full of interest.

The trichogyne of Nemalion contains a well defined nucleus which appears shortly after this structure begins to push upward from the carpogonium, but is only found in a fragmented condition in the mature trichogyne. The egg nucleus, which lies above the chromatophore in the carpogonium during the early development of the trichogyne, passes later to the bottom of the carpogonium. Although no mitotic figure was observed it seems clear that the nuclei of the trichogyne and egg are sisters, following a division in the terminal cell of the procarp.

These observations on Nemalion confirm my account of the trichogyne

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[1] WOLFE, J. J., Cytological studies on Nemalion. Ann. Botany **18**:607–630. *pls. 40–41.* 1904.